cult to see from the drawing what is meant in the arrangement figured between the washing bottle and the French drying tube. There are some traces also of careless printing, which it would be well to rectify in future editions, as in the equation of the action of arseniuretted hydrogen on silver nitrate on p. 372. The title of the book is also somewhat presumptuous; it is styled "A Manual of Practical Chemistry:" the two last words being in large type; a colon is here introduced and then follows the exact title of the book in smaller type, "The Analysis of Foods and the Detection of Poisons." The work cannot be fairly described as a Manual of Practical Chemistry, and the title should therefore have been restricted to the matter actually contained in the book.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

### On the Spectrum of Brorsen's Comet

WITH reference to Prof C. A. Young's Note on the Spectrum of Brorsen's Comet, in NATURE, vol. xix. p. 559, it may be of interest to mention that observations made at the Royal Observatory, Greenwich, confirm his conclusion as to the coincidence of the brightest band in the comet spectrum with the green band of carbon.

We were not able to examine the comet's spectrum till April 17, as the Great Equatoreal was in the workmen's hands till that date for alterations required to allow of the more convenient use of the spectroscope. On that evening, and again on April 19, the comet's spectrum was repeatedly compared by Mr. Maunder and myself, with the spectrum of alcohol taken in a vacuum tube. The less refrangible edge of the brightest comet-band coincided as exactly as could be determined with the corresponding edge of the green carbon-band at 5,200, but the comet-band was very much wider, extending two-thirds of the way towards F (*i.e.*, about 200 tenth-metres), and covering the carbon-band at 5,200 (about 30 tenth-metres broad) and the two following fainter bands at 5,100 and 5,020. The comparisons were made on April 17 by the help of an occulting bar, and on April 19 with Hilger's bright-line micrometer, illuminated by red light. With the latter, readings for the comet- and carbon-bands respectively, agreed within half a tenth-metre. The half prism spectroscope with a dispersion of 10° from A to H (equivalent to two prisms of 60°) was used on the 13-inch equatoreal. From spectroscopic observations of the carbon compound, printed in the volume of Greenwich Observations, 1875, it appears that the bands in the spectrum of alcohol are identical with those in the spectra of olefiant gas, and of carbon oxide and dioxide.

A second band was seen in the orange of the comet's spectrum approximately coincident with the carbon band at about 5,600. This band was of about one-fourth the brightness of the principal band.

The results on April 17 were obtained without a knowledge of Prof. Young's work, and thus afford an independent confirmation of his conclusion. W. H. M. CHRISTIE

Royal Observatory, Greenwich, April 21

### Blue Flame from Common Salt

I AM perfectly aware that, as Dr. Gladstone points out in your last issue, I have not *proved* HCl to be the origin of the blue flame, but I will give some of my reasons for *thinking* so.

In the first place I conclude every one will admit that chlorine in some form must be present, since only chlorides produce the flame. At one time I thought it was due to dissociated or atomic chlorine; that view, however, I discarded in favour of the hydrochloric acid theory.

When AmCl is heated, dissociation occurs, as is well known, $NH_3$ and HCl being formed; the HCl then plays its part in producing the blue flame. If calomel be used, it is natural to imagine that the mercury and chlorine are separated, and if the colour is due to HCl, the addition of hydrogen will be necessary before the flame is produced. As a matter of fact I have found that no coloration occurs if the calomel is heated in what I may perhaps be allowed to call the *solid* part of the Bunsen flame, *i.e.* where complete combustion takes place, but it is necessary to allow some of the unburnt gas to mingle with its vapour. In practice I adjust the wire gauze over the burner so that a black spot is seen surrounded by a red hot ring, a little calomel placed on the dark spot volatilises and colours the gas that is burning above the gauze; if the gauze is raised so that the dark spot vanishes and all is red hot, the salt volatilises without any coloration ensuing.

Although I have not been able to see any violet bands when a spark has been taken in HCl, I do not consider that it negatives my theory, since there is a considerable difference between an electric spark and a Bunsen flame, and I now have reason to think that under the influence of the spark the HCl is split up into its components, which will fully account for the absence of violet bands. I have likewise failed to get them from a spark in AmCl.

A drop of liquid HCl, introduced into a Bunsen flame by the aid of a platinum wire, gives a flash of blue colour, and a lighted taper immersed in a bottle of HCl gas has its flame surrounded by a blue mantle just before it goes out. The colour, to the eye, is identical in both cases to that produced by the volatilisation of a chloride, the peculiar violet tinge showing that it must contain rays of high refrangibility.

Lastly, if a stream of HCl gas be slowly passed into a large Bunsen flame, the colour is produced most vividly, the spectrum showing all the characteristic lines or bands. Here we have the HCl under the same conditions as the chloride and with a similar result.

Dr. Gladstone appears not to have obtained the flame by this method, since he says: "Hydrochloric acid passed into a flame never gives the violet light."

This may probably be explained by the fact that if the HCl be passed too rapidly the violet coloration gives place to green, similar to that produced by chlorine alone if the stream of gas be allowed to slacken, the violet is reproduced, and this may be repeated indefinitely. A. PERCY SMITH

Temple Observatory, Rugby, April 26

### Did Flowers Exist during the Carboniferous Epoch?

ACCORDING to the position Mr. Wallace has taken in the discussion as to the order of insects to which *Breyeria borinensis* presumably belongs, everything depends upon the existence or non-existence of transverse reticulation. I re-assert that a regular and thoroughly well-marked transverse reticulation exists over all the wing.

If Mr. Wallace prefers to believe in the evidence afforded by a photograph in preference to my statement based upon actual examination, and to M. de Borre's words in his description ("Entre toutes ces nervures s'étend un réseau extrêmement complet de très-fines nervules allant transversalement d'une grosse nervure à l'autre"), it is evident that anything I could say would not alter his opinion.

Further, I utterly fail to comprehend by what process of reasoning he arrives at the conclusion that the photograph "is so beautifully sharp that it brings out the minutest details," when confessedly he has not compared that photograph with the original.

That the main nervures may be compared with some forms in Lepidoptera and found to agree to a certain extent is very possible; it would be singular if it were otherwise, considering the extreme variation in the neuration of Lepidoptera, and the practical certainty that the system of neuration in all orders of insects can be homologised. The presence of dense *transverse* reticulation in a lepidopterous insect would decidedly be an anomaly; but its absence would not prove that any particular fossil *did not* belong to the Ephemeridæ, for in some recent genera of the latter, such as Oligoneuria, Lachlania, &c., the transverse reticulation is so far absent as to be reduced to a few nervules that might be counted on the fingers of one hand.

Supposing, for the sake of argument, that my assertion may be based upon false premises (and no one is infallible), *Breyeria* would probably be relegated to that heterogeneous assemblage of extinct forms of insects possessing densely reticulate wings, to

accommodate which the order Palæodictyoptera has been formed. It is not for me to here enter into an examination of the materials included in this so-called order. It will suffice to say that not one of them could be suspected of being lepidopterous.

The point at issue is, did anthophilous insects (and therefore flowers also) exist during the carboniferous epoch? According to my views we are without evidence of their existence.

I decline any further discussion on this subject until Mr. Wallace has examined the fossil, or has obtained evidence of its peculiarities from some one in whose judgment he has more confidence than he apparently has in mine.

Lewisham, April 25 R. McLACHLAN

## Captain Cook's Accuracy

IN NATURE, vol. xix. p. 408, there is an article entitled "Captain Cook's Accuracy," which I think reflects unjustly upon the late Admiral Wilkes, U.S.N. As a specimen of Wilkes's inaccuracy the writer of the article cites first the discrepancy in the position of Turtle Island, the south-easternmost of the Fiji group, Cook and Wilkes differing more than 30′ of longitude. The narrative of the U.S. Exploring Expedition was written on board ship during the progress of the work, and was placed by Wilkes in the hands of the printer immediately upon his return, in order that the general results might be known without delay. The astronomical positions were given as they were recorded at the time, and were not corrected for final chronometric errors and rates, which were carefully ascertained while the charts were being prepared for publication. A comparison of the narrative with the atlas, published subsequently, will exhibit differences of longitude almost throughout.

On the general chart of the Pacific, sheet III., which is on a very small scale, so that a slight inaccuracy of the draughtsman or engraver will cause a difference of several minutes, Turtle Island will be found to be in about 178° 22′ W. long., but the special plan of the island (vol. 2, p. 94, of the Atlas) places it in lat. 19° 47′ S., and long. 178° 16′ 18″ W., while Capt. Denham, R.N., in 1856, places it in 19° 49′ 11″ S., and 178° 14′ 42″ W., where it is at present shown on the British Admiralty Charts. The difference of latitude is about 1′ 45″; that of longitude, 1′ 36″; differences which might readily be accounted for by different points of observation having been used. The difference in the outline is not very material.

As Cook placed the Island in 178° W., he was fifteen minutes in error; while Wilkes differs from the latest surveys about a minute and a half. Capt. Worth, of H.M.S. *Calypso*, in 1848, placed the island in 178° 8′ W., differing seven miles from the subsequent survey by Capt. Denham, the position by the latter being now borne on the British Admiralty chart, yet the former authority is quoted to prove the inaccuracy of Wilkes's work.

Findlay, judging from what he says upon this subject, consulted Wilkes's book, instead of his chart, which was published subsequently. The second example of Wilkes's inaccuracy, cited by the writer, is that he found from a position which he occupied at Savaii, a trend of coast differing from that as shown by Wilkes's chart, but it is a question whether he was not mistaken in the identity of the point occupied by him. The waters of the Samoan group are, so far as we know, navigated safely and almost exclusively with Wilkes's charts.

The third and last example is concerning Quiros Island (Swain's Island). The facts in this case are that the boats of the exploring expedition did *not* effect a landing on the island at all; efforts were made to do so, but were unavailing on account of the surf, so that it is quite impossible that they could report the existence of a lagoon hid from their view by a wooded strip of land even if only a quarter of a mile in width.

In criticising the work of such explorers as Cook, Vancouver, and Wilkes, it should be borne in mind that the expeditions which they commanded were for exploring rather than surveying purposes, and it is rather a matter of surprise that they should have come so near the truth when we consider the crude materials with which they had to work. S. R. FRANKLIN

Hydrographic Office, U.S. Navy, Washington, D.C., April 11 — Captain U.S.N. and Hydrographer

## Sense of Force and Sense of Temperature

"J. T. B.'s" "discovery" of the distinction between muscular sensations—or, as he styles them, the "sense of force," whatever that may mean—and the sensation of temperature, has been long anticipated by Alexander Bain in his work on "The Senses and the Intellect."

Again, your correspondent's illustrations of the distinction he draws between absolute and relative muscular sensations and sensations of temperature are wholly illusory. How can it be said that a letter-sorter enjoys and improves absolute sensations of weight? Surely his sensations enable him to determine not "absolute weight" (whatever that may be), but the weights of particular letters relative to certain standards, according to which relation the postage is charged. These sensations enable him to say that certain letters are over, and others under, an ounce in weight, and thus they are in fact relative, not absolute, as "J. T. B." seems to suppose.

The same remarks apply to "J. T. B.'s" assertion that "the sense of temperature may also be rendered absolute to a certain extent," and to his illustration of the plumber who judges whether the heat of the soldering-bolt is adequate for his purpose. Here again the sensations are, in truth, purely relative, any inference drawn from them being based upon a comparison of present and previous sensations and present and previous experience of their results. A. K. R.

Mark Lane, April 23

## Mr. Preston on General Temperature-Equilibrium

MY attention has been arrested by Mr. S. Tolver Preston's paper on general temperature-equilibrium in NATURE, vol. xix. p. 460, and by a letter from him in a later number (p. 555), pointing out a trifling literary ambiguity in it. As this implies that the paper is otherwise correct, you will perhaps allow me to protest, and to state that it is full of confusion of reasoning and of unsoundness.

I do not know how many sins against dynamics could be discovered by careful examination, but at least two pervade it throughout, viz. (1), the assumption that the simple relationship which exists between the movements and the temperatures of molecules of matter exists also between the movements and the temperatures of masses of matter; (2) the assumption that gaseous molecules (simple or compound) whose bond is chemical affinity differ mechanically from masses of matter (stellar or otherwise) in size and weight only, whereas they really differ in a multitude of other ways, and notably in elasticity; and from this difference alone it would be easy to show that the analogy in the paper is fanciful, and its reasonings and conclusions invalid, but I respect your space.

In conclusion I would say that I am not writing against the hypothesis of temperature-equilibrium itself. It may or may not be true. All I assert is, that this paper gives no real information about it. WM. MUIR

133, Upper Thames Street, E.C., April 26

## The Migration of Birds

IT was because Prof. Newton mentioned such distances as six, seven, and ten miles (*vide* NATURE, vol. xix. p. 434), in connection with the flight of migratory birds, that I brought forward the matter of temperature, and the latter still appears to me to have as much bearing on the question, as has the density of the atmosphere.

The intense frost on Christmas eve, 1861, was said to have killed a large number of thrushes, blackbirds, &c., in Scotland. Near Edinburgh, where the thermometer registered about − 4° F. during the night, many dead birds were found. These deaths resulted from cold, not from starvation, for the weather was open until within a few days of Christmas day. Now, if a frost of this severity has such an effect on bird-life, surely it must be conceded that temperatures from − 25° to − 100° F.—those that would reign between six and ten miles' elevation, with surface temperature of + 80° F.—would slay the hardiest migrant.

There is a great difference between the elevation required to view a distant sea horizon, and an equally distant mountain-top. For instance, to obtain a sea-horizon of 300 miles, you must mount nearly twelve miles; but from an altitude of four miles, the summit of a mountain 20,000 feet high (less than 4 miles) would be visible, though its base lay 300 miles off. Similarly, if an elevation of 5,000 feet only be granted to the haze that constitutes the loom of land, birds flying two miles high will have a circle of vision, for the land indication, of over 200 miles radius. Under such circumstances, if the journey is 1,000 miles in length, a deviation of some 12° on either side of the true direction of flight